# Effects of Welfare Sanctions in Couple Households

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# DISCUSSION PAPER SERIES

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ABSTRACT

# Effects of Welfare Sanctions in Couple Households*

Means-tested welfare benefits are usually provided at the household level. Job search effort of unemployed welfare benefit recipients is monitored, and non-compliance with job search requirements can lead to a sanction and therewith to a temporary drop in household income. Among unemployed couples on welfare, a sanction is typically induced by one of the partners but potentially the burden is shared by both. We consider effects of sanctions on their transition rates into work. We examine theoretical implications and provide empirical evidence based on administrative data from Germany. We find that sanctions increase the probability of entering employment for the sanctioned welfare recipient but also for their partner. Females react more strongly to a sanction of their partner than males.

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# 1    Introduction

Welfare benefits are usually provided at the household level. Unemployed welfare recipients must meet specific job search requirements, and failure to comply can result in sanctions, leading to a substantial reduction in household income. In a household with two unemployed welfare recipients, a sanction may be triggered by one of them, but the income reduction may affect all household members. Welfare benefits are typically means-tested, and households have limited capacity to smooth consumption following an income drop. In response to this financial shock, possibly accompanied by increased monitoring, both individuals may adjust their job search behavior. The existing literature on sanction effects focuses on the employment outcomes of the sanctioned person, notably on the transition rate from welfare to work, ignoring potential effects on other household members (Pattaro et al., 2022).

In the present paper, we jointly analyze effects on transitions to employment for both the sanctioned individual and their partner. This provides a comprehensive account of sanction effects on the time until the household moves out of welfare into wage dependency, which is a necessary ingredient for any social-welfare evaluation of the welfare program. The analysis also provides insights into the extent to which the partners' genders affect the ways in which a sanction plays out. In addition, the paper presents a methodological innovation by incorporating statistical interdependencies across partners in the rates at which they obtain a sanction and move to work, as well as causal effects of one of such events on the rate at which the partner experiences an event. From an econometric point of view it is essential to account for these interdependencies, to avoid selection and misspecification biases.

Standard theoretical job search models for single individuals predict a faster transition to work once a sanction is imposed, since the value of unemployment decreases and hence the search intensity rises (Abbring et al., 2005). In the paper, we extend this theoretical framework to a setting with couples of welfare recipients. Here, monitoring is concerned with individual job search efforts, while a sanction-induced benefit reduction affects the joint household income. It turns out that this extension complicates the theoretical analysis substantially. In particular, effects of monitoring and sanctions of the *partner* on an

individual's transition rate to work can differ profoundly from the effects of monitoring and sanctions of the individual under consideration. If a partner's search costs are not internalized in the individual's decision-making, it may even be in the individual's interest for their household to receive a sanction. If the partner faces high search costs but also has high earnings potential, the individual under consideration might want to reduce their own search effort, inducing a violation so they can ultimately benefit from the higher earnings of the partner. This ex-ante effect of sanctions has the opposite sign compared to the standard case. We further show that in such a setting, under certain conditions, the transition rate to employment for one individual may decrease following a sanction induced by the behavior of the other. This outcome is particularly relevant when search costs differ significantly between partners and when one partner meets the search requirements before the sanction is imposed.

The empirical analysis is based on a large sample of households entering welfare in Germany, drawn from administrative records, and followed over time. The study design is inherently dynamic in the sense that sanctions are not known in advance and may arrive at various times, and realized sanctions only affect behavior and outcomes from their issuance onwards. As has been argued by, e.g., Abbring et al. (2005), unobserved confounders are an essential feature of evaluation studies of sanctions. We therefore estimate multivariate duration models taking account of the dynamic selection of each partner into treatment as well as into employment. This includes the cross-effect of the partner's sanction on the transition rate to the own sanction. The latter sheds light on whether welfare recipients are more willing to comply with the legal requirements after a sanction, an important intermediate outcome of the effect of sanctions on labor market outcomes. Auxiliary justification of underlying model assumptions is obtained from interviews we conducted with caseworkers from various job centers, regarding the institutional setting and the implementation of guidelines and requirements by caseworkers. In a heterogeneity analysis, we deal with the exceptional position of couples with small children, where one of the parents is exempt from monitoring and sanctions.

Our study contributes to research on benefit sanction effects. Van den Berg et al. (2004),

Van der Klaauw and van Ours (2013), and Busk (2016) consider unemployed welfare recipients (ignoring partner sanctions) and find positive sanction effects on their own transition rate to work. Our study can also be related to a number of other bodies of work; specifically those focusing on gender differences in behavior and outcomes. First, a range of average active labor market policy effects is known to vary with the gender of the treated individual (Bergemann and van den Berg, 2008; Card et al., 2018). Regarding sanction effects on the individual's transition rate from welfare to work, van den Berg et al. (2004) do not find gender differences, whereas van der Klaauw and van Ours (2013) find larger effects for women and Busk (2016) finds larger effects for men. A recent study by Chan et al. (2024) shows that a reform in Australia that increased job search requirements for welfare recipients decreased the proportion of mature-aged partnered women on welfare as well as the proportion of their male partners on welfare.

Another relevant body of work examines transition rates to work of household members if one of them experiences a negative shock in the labor market. For example, García-Pérez and Rendon (2020) provide evidence for an added worker effect in a model of family job search: a job separation for one partner leads to an increase in the job finding rate for the partner. A related and much wider literature examines how a sudden change in household income or household assets is allocated between spouses. Vaessen et al. (2014) perform a meta-review of the effects of the provision of microcredit to women on the degree of their empowerment and their control over household spending in developing countries. They conclude that there is no evidence for nonzero effects. Indeed, women themselves appear not to benefit from microcredit in the short run and may even suffer if there are no fallback options outside marriage; see, e.g., Balasubramanian (2013). In our punitive sanction setting, this might suggest that women might on average suffer more from a punishment than men.

The microcredit literature also argues that women are more adept than men in using insurance benefits in order to make profitable investments, and that women tend to follow the insurance guidelines and make any mandatory payments on time. Of course, welfare is not an insurance scheme, but the monitoring aspect makes it share features with insurance systems.

Other related studies consider how the outcome of a dictator game depends on the gender of the participants. Meta-analyses by Doñate-Buendía et al. (2022) and Bilén et al. (2021) conclude that women are more generous than men on average, in particular if the recipient of the game is male (so has a gender opposite to the giver).

Much of the literature in the above paragraphs focuses on the relative bargaining power and personality traits of partners, as represented by their gender. We show that in our setting, effects of a sudden household income change cannot be straightforwardly explained by differences in intra-household bargaining power but may depend on each spouse's individual labor market opportunities, personal search costs, and institutional regulations. Furthermore, in our setting, the moment of income change is inherently unknowable in advance, and each partner is at risk of a punitive sanction both before and after the other partner is punished. Nevertheless, we view the cited literature as relevant in drawing attention to the determinants of intra-household bargaining.

The paper is organized as follows: Section 2 describes the institutional background. Section 3 discusses theoretical models of unemployed couples receiving welfare benefits and facing the risk of benefit sanctions. Section 4 describes the data, the sample selection, and presents some descriptive statistics. Section 5 presents the econometric approach. The results of the empirical analysis are presented in Section 6, and Section 7 concludes.

# 2    Institutional background

This section describes the means-tested benefit system and benefit sanction regulations in place during our observation period from 2008 to 2015. The monitoring and sanction policies are implemented by caseworkers who have some discretionary power. To understand the range of possible actions that the caseworkers can take, we rely on qualitative interviews with 12 caseworkers.[1]

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[1]The interviews were conducted by telephone with caseworkers employed in different job centers. The selection of interview locations ensured that heterogeneous job centers in terms of the local rate of welfare receipt, the job center sanction rate, and the regional and labor market structure were included. The main topics of the interviews were the process of implementation of sanctions, the documentation of sanctions in administrative systems in the job centers, and the expected effects of own and partner sanctions in the period before November 2019. In November 2019, sanction regulations changed following a ruling of the German Federal Constitutional Court. This paper focuses on sanction regulations in the period before November 2019.

Welfare benefits are means-tested at the household level. Individuals who are capable of working receive "Unemployment Benefit II" (UB II, or *Arbeitslosengeld II*), consisting of two main components. First, a basic cash benefit covers day-to-day expenditures. The amount varies by the household composition. In our observation period, each partner in a household receives an amount ranging between €316 in mid-2008 to €360 in 2015. For each child in the household, an additional amount is added to the basic cash benefit, depending on the age of the child.[2] Second, costs for accommodation and heating are covered.

UB II recipients have to comply with specific requirements related to their labor market integration. A violation of these requirements can lead to a sanction. These requirements concern—next to a general cooperation with the job center—their job search behavior and potential participation in active labor market policy measures (ALMP). In households with children, welfare recipients may face different requirements. In particular, those caring for children below the age of three are not required to take up employment or participate in ALMP. However, the partner of the caretaker is still required to comply with the requirements.[3]

If welfare recipients with children below the age of three choose to rely on childcare facilities rather than to provide the care themselves, they will be required to take up employment and participate in ALMP. For children aged three and older, the requirements the welfare recipients have to comply with depend on the availability of childcare, such as access to childcare facilities.

If welfare recipients do not comply with their obligations, they can be sanctioned for a duration of 3 months. These sanctions affect the cash benefit of the person who does not comply with the rules. Comparatively mild sanctions imply a reduction of the basic cash benefit by 10%, and the main reason for a mild sanction is missing an appointment with

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[2]In addition to partners, other household members receive basic cash benefits. The amount depends on the member's age and, in 2008, ranged from €211 for those under 14 to €281 for those aged 14 and older. Between 2008 and 2015, these amounts increased, and additional age categories were introduced. In 2015, the basic cash benefit for household members other than partners ranged from €234 to €320.

[3]The couple decides which of the two is responsible for childcare and who is going to follow the search requirements. This decision is conveyed to the caseworker. It is possible for spouses to change roles. This would have to be coordinated with the caseworker, because the caseworker has to formally exempt the person with childcare responsibilities from search requirements. The only restriction is that both spouses cannot claim being exempted from the search requirements in the same period.

the caseworker. The refusal to accept a job offer, to search for a job, or to participate in an ALMP can lead to a strong sanction. The first infringement leads to a reduction by 30% of the basic cash benefit. The second infringement within one year leads to a 60% reduction, and any further infringement within one year to a complete withdrawal of UB II, including the costs for accommodation and heating.[4]

The process of imposing a benefit sanction involves several steps (van den Berg et al., 2022): as soon as individuals register for welfare benefits, the job center has to inform them about their obligations and the consequences of non-compliance. If welfare recipients do not comply with these rules and the job center observes such an infringement then the caseworker has to document this. The welfare recipient receives a written notification containing the details of the infringement and the sanction. He or she can reply to this with a reply form, which is part of the notification letter. In the qualitative interviews, some caseworkers explain that they offer to have a personal meeting with the welfare recipient. The welfare recipient has a deadline of around 2.5 weeks to respond (according to the interviews we carried out). If the welfare recipient can not bring forward a good cause—e.g., sickness or childcare obligations—within the specified deadline, then, shortly after this deadline, the caseworker informs the colleague responsible for payment of UB II to send a second notification which includes the date of the imminent sanction. The job center usually sends this notification in the month preceding the beginning of the sanction. The time interval before the start of the sanction is usually one to three weeks, as information on the date of registering a sanction from another data source and the actual start of the sanction in our data suggest.[5]

An infringement does not always lead to a sanction (van den Berg et al., 2022). This may be for various reasons: first, caseworkers with a high caseload do not have the time to monitor all welfare recipients with equal scrutiny. Second, if infringements are not fully observed and therefore cannot be well documented, the caseworker might not impose a

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[4]For welfare recipients below the age of 25 years, the first infringement reduces UB II to cover only costs for accommodation and heating. Another infringement within one year leads to a complete withdrawal of UB II. In this paper, we focus on individuals aged at least 25 years.

[5]The welfare recipient can formally object or bring the case to a court, for example, if the job center made a formal mistake. However, this does not stop the sanction automatically. Such a formal complaint can be issued up to one month after receiving the second notification, which may be after the sanction was already in place.

sanction as to not risk a lawsuit. Some interviewed caseworkers mention that they decide in favor of welfare recipients in a case of doubt of whether there is sufficient proof of an infringement to avoid objections or lawsuits. Third, the definition of a good cause for non-compliance in the benefit rules is legally not fully defined. Qualitative studies suggest that sanctions are not universally imposed (Götz et al., 2010). Some caseworkers try to avoid imposing a benefit sanction in situations where a sanction would actually be possible (Karl et al., 2011). Our caseworker interviews confirm this, suggesting that caseworkers have room for discretion to evaluate what constitutes a good cause and whether to impose a sanction.

During our observation period, an annual average of 4.6 million welfare recipients capable of working are part of 3.4 million households receiving welfare benefits. Around 26% of these households are couple households. According to data from the Statistics Department of the Federal Employment Agency, the annual number of imposed sanctions in this period ranges from 0.73 to 1.01 million between 2008 and 2015. The number of strong sanctions per year remains fairly constant from 2008 to 2012 and decreases from 318.500 in 2012 to 229.669 in 2015. The sanction rates—defined as the share of welfare recipients capable of working with at least one sanction that is in force in a calender month—are on average 3% and range between 2.5% and 3.4%. The sanction rate of men tends to be approximately double the sanction rate of women and is on average 4.2%.

# 3 Theoretical considerations based on sequential job search models with monitoring and sanctions

This section discusses theoretical models of unemployed couples that receive welfare benefits in a labor market in which job search effort is monitored and sanctions can be imposed. These are sequential job search models and capture some key features of the German welfare system. The focus is on effects on transition rates to work of each member of the couple.

**Single agent.** A natural starting point is the search model in Abbring, van den Berg and van Ours (2005) for a single individual and with undispersed wage offers. Job offers arrive at an unemployed individual $i$ according to the rate $\lambda_i \cdot s_i$ where $s_i$ is the search effort made by

$i$. The jobs pay a wage $w_i$ which is the only job characteristic. We assume that once a job is accepted it will be held forever at the same wage. During unemployment, a flow of benefits $b_i$ is received. While searching, search costs are made that depend on effort according to the function $c_i \cdot s_i^2$.

Monitoring targets search effort $s_i$. If the effort is deemed insufficient then a sanction is imposed. The sanction means (i) a fraction $1 - \alpha$ of the benefits is taken away from the individual and (ii) the individual henceforth exerts a search effort that is deemed admissible. Condition (ii) is motivated by heavily increased monitoring after a detected violation and a draconian punishment for recidivism.

For a given choice of $s_i$, the transition rate to employment equals $\lambda_i \cdot s_i$ and the sanction rate is $\eta_i(s_i)$ as determined by some decreasing function $\eta(.)$.

Consider an individual at risk of a sanction. As shown in Abbring, van den Berg and van Ours (2005) the model predicts: (1) upon a sanction, the transition rate to employment increases, (2) the transition rate to employment before a sanction is imposed is higher than in a market without monitoring, and (3) the sanction rate and the transition rates to employment before and after a sanction all depend on individual characteristics and his/her labor market and institutional environment. Predictions (1) and (2) constitute the so-called *ex-post* and *ex-ante* (or the *treatment* and *regime*) effects of a sanction, respectively. Prediction (3) implies that selectivity is an inevitable feature of any econometric inference.

**Couples.** Now consider a couple with members $i = 1$ and $i = 2$. They receive a joint unemployment (or welfare) benefit $b$, and they share this amongst each other according to fractions $1 - \gamma$ and $\gamma$, respectively. If one of them gets a job then in effect the other does not receive benefits anymore, and they end up sharing the wage income instead. Of course, the individual who is still unemployed may continue the job search so that in the end the total household income may consist of the sum $w_1 + w_2$. We abstract from savings and household dissolutions.

In this setting, monitoring targets both search efforts $s_1$ and $s_2$ separately. However, a sanction potentially also affects the partner of the perpetrator. This is, first of all, because a

fraction $1-\alpha$ of the joint household benefit is taken away. Secondly, monitoring of household activities increases. We assume that after a sanction, both individuals exert a search effort that is sufficiently high to prevent a further sanction. Thus, after a sanction is given, it is irrelevant which of the two partners was the perpetrator.

Although this may seem a straightforward extension of the above single-agent model, the ensuing analysis of optimal behavior and resulting transition rates is not transparent. An individual unemployment spell can now witness up to three events, namely a sanction, a job acceptance by the partner, and the own job acceptance. Every decision depends on the decisions of the partner and on all determinants of possible future events, including the characteristics, environment, and employment status of the partner. The model equations for optimal search efforts are highly nonlinear. The literature contains models that allow for multiple search endeavors taking place concurrently, but these are not straightforwardly amenable to our setting. Notably, van den Berg and van der Klaauw (2006) consider the usage of two types of search channels (formal and informal) where the effort along only one of these (formal) is monitored and sanctioned. Guler et al. (2012) study job search by couples that take geographical locations into account but does not consider monitoring and sanctions.

With this in mind, we provide some insights derived from different model varieties. A key distinction is between what, with some abuse of language, can be called cooperative and non-cooperative models. In the latter, partners maximize their own expected present value without taking the search costs into account that the partner incurs. We assume that the sharing of the contemporaneous income flow in the household always occurs according to the parameter $\gamma$. Regarding household search costs $c$, we assume for now that these are additive across partners, so

$$c(s_1, s_2) = c_1 \cdot s_1^2 + c_2 \cdot s_2^2 \tag{1}$$

Consider the case where both partners are unemployed without having been given a sanction. Using the asset flow representation of the Bellman equation, the expected present value $V_1$

for $i = 1$ in the non-cooperative setting can be expressed as

$$\rho V_1 = \max_{s_1} b_1 - c_1 s_1^2 + \lambda_1 s_1 [V_{1a} - V_1] + \lambda_2 s_2 [V_{1b} - V_1] + (\eta_1(s_1) + \eta_2(s_2))[V_{1c} - V_1]$$

for a given effort $s_2$ (which we assume is observable to individual 1). Here, $\rho$ is the discount rate, $b_1 = (1 - \gamma)b$, and $V_{1a}, V_{1b}, V_{1c}$ are the expected present values of individual $i = 1$, associated with (a) $i = 1$ having a job while $i = 2$ is unemployed and may receive a job later, (b) $i = 2$ having a job while $i = 1$ is unemployed and may receive a job later, and (c) having received a sanction. The latter three values and the values for $i = 2$ can be expressed in a similar way. In the non-cooperative setting, individual $i$ chooses $s_i$ to maximize his/her own value given $s_j$ and vice versa. The equilibrium is characterized by the solution of this system of two equations in $s_1$ and $s_2$.

As noted, our focus is on the transition rates to employment. We denote these by $\theta_{e1}$ and $\theta_{e2}$. Analogous to the case of a single agent, they are equal to $\lambda_1 \cdot s_1$ and $\lambda_2 \cdot s_2$, respectively. The sanction rates are $\eta_1(s_1)$ and $\eta_2(s_2)$. As in the single-agent case, the optimal current search efforts and the ensuing sanction rate and the transition rates to employment before and after a sanction all depend on characteristics of the individual and his/her labor market and institutional environment (except that the sanction rate is zero after a sanction). In the case of a couple, these rates may also depend on the characteristics, environment, and employment status of the partner. This implies that from an econometric inference point of view, it is essential to allow for related confounders across partners, as well as across the hazard rates for a given partner. Some of these confounders may be unobserved. Indeed, as explained by Abbring, van den Berg and van Ours (2005), in a setting with punitive sanctions, unobserved confounders cannot be ignored, since the enforcement of guidelines would be straightforward if they are fully driven by observed characteristics.

As in the case of a single agent, one may define ex-ante and ex-post effects of sanctions. Bear in mind that the focus here is on effects on transition rates into employment (and not on expected present values or search efforts per se). For each partner, there are two ex-ante effects, depending on whether the regime refers to oneself or to the partner. Each partner also faces two ex-post effects. Upon receiving a sanction, it is immaterial who triggered it,

but the sanction can have different effects on $s_1$ and $s_2$, so the transition rates of the partners may be affected to a different extent. Also, one may envisage model extensions where the partner responsible for the sanction experiences harsher monitoring after the sanction than the other partner, potentially leading to ex-post effects for a given partner that depend on who triggered the sanction. We return to this later.[6]

A monitoring system has a threshold $s^*$ such that a search effort that exceeds it prevents sanctions. Consider a couple where both partners are susceptible to a sanction, so $s_1, s_2 < s^*$. In this case, a sanction always leads to an increase in $s_1$ and $s_2$ up to $s^*$, and hence to increases in the couple's transition rates to employment $\theta_{e1}$ and $\theta_{e2}$. This setting could be labeled as the default extension of the single-agent setting.

In the non-cooperative model, the search costs made by the partner are not internalized into one's own decisions. This leads to the fascinating insight that in certain cases it can be in the interest of one of the partners that the other one gets a sanction. To see this, consider an example where $i = 2$ has a prohibitively high unit search costs $c_2$ but also has excellent labor market opportunities as captured by high values of $\lambda_2$ and $w_2$. Due to the high $c_2$, this individual does not exert search effort, so $s_2 \approx 0$. Conversely, let partner $i = 1$ have a low $c_1$ and also have low values of $\lambda_1$ and $w_1$. Upon imposition of a sanction, individual $i = 2$ is forced to increase effort to $s^*$, and this will soon lead to a job acceptance by this individual, with a high income $w_2$. This in turn benefits individual $i = 1$, as the joint income is shared between them. With $s_1, s_2 < s^*$, the sanction still leads to an increase in the transition rates to employment for each partner, just like in the default case. However, individual $i = 1$ now has an incentive to reduce the own effort $s_1$ prior to a sanction with the aim to induce a sanction. Indeed, $i = 1$ has an incentive to report his/her partner's violation of the search effort rules to the monitoring agency. Effectively, this is an ex-ante effect with a different sign than in the single-agent case.

We now consider a slight modification of the example, where initially $i = 2$ behaves the same as above (so $s_2$ is small) but $i = 1$ faces conditions that actually make him/her

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[6]Of additional interest are effects of job acceptance of one partner on the transition rate to work of the other (entailing loss of welfare benefits). In the empirical analysis, the durations in welfare are right-censored at such events, so we do not consider these effects here.

initially choose $s_1 > s^*$. We focus on the ex-post effect for $i = 1$. Note that an imposed sanction is now necessarily due to the partner $i = 2$. In general, the default implication of a sanction that was triggered by the partner appears to be that $s_1$ increases further, in order to deal with the loss of welfare benefits. However, in the specific example, the expected present value of unemployment for $i = 1$ could actually increase upon the sanction, because it makes lucrative employment of the partner $i = 2$ much more likely in the short run. The sanction may then incentivize $i = 1$ to reduce his/her effort $s_1$ after the sanction, and this would translate into a lower transition rate to employment for $i = 1$. In an extreme setting, the individual $i = 1$ may even prefer to withdraw from the labor market. The example therefore shows that, under certain circumstances, the transition rate to work can decrease if the partner's behavior triggers the imposition of a sanction. Such effects may be empirically relevant if the unit search costs differ dramatically between the partners. If the partner experiences a very high disutility of job search due to being the caretaker of many children in the household then this may explain a high $c_2$. We return to this below.

The analysis so far provides some novel insights that are important for the empirical analyses. First, econometric inference on ex-post effects needs to address unobserved confounders of the sanction rates and the transition rates to work that are statistically related within and across the partners in the household. This restricts the range of methods that can be applied. Secondly, the effects of monitoring and sanctions of the *partner* on an individual's transition rate to work can be profoundly different from the effects of monitoring and sanctions of the individual under consideration. This complicates the formulation of simple hypotheses on the signs of the effects of monitoring and sanctions of the partner. Thirdly, and related to this, the connection between intra-household bargaining power on the one hand, and the effect of a sanction on the partners' employment outcomes on the other hand, is indirect, complex, and fundamentally dependent on the individual labor market conditions of the two partners. The intra-household bargaining power parameter $\gamma$ is only one among a range of determinants of the employment outcomes. Specifically, labor market conditions $\lambda_1, w_1, \lambda_2, w_2$, personal search costs $c_1, c_2$, and institutional conditions $\eta_1, \eta_2, b, \alpha, s^*$ all influence behavior and who ends up with what, in a highly non-linear

fashion. This third insight is of importance for the interpretation of the estimated ex-post sanction effects. Those estimates do not translate straightforwardly into statements about the relative bargaining power of the partners. One could argue that this insight is a special case of a more general principle, namely that in markets with imperfect competition and some degree of non-cooperativeness in the household, the actual within-household divisions reflect not only bargaining power but also differences in individual labor market opportunities. In addition, it follows that, from an econometric point of view, ex-post effects are plausibly heterogeneous, depending on conditions in the household.[7]

**Other model versions and extensions.** So far we have assumed that search costs are not directly shared but the instantaneous household income stream is always divided according to $\gamma$. One may envisage even less cooperative settings, where the share of this stream that one of the partners receives in unemployment is not reduced by a sanction if the sanction is triggered by the other, so that the financial burden of such a sanction is always borne by the partner. In such cases, the transition rate to work of one of the partners may be insensitive to a sanction for the partner.

This line of thought may be elaborated on by considering the determinants of the bargaining power parameter $\gamma$. Balasubramanian (2013) provides an interesting game-theoretic analysis in the setting of micro-credit provision in South-Asia, to address the question why economic outcomes of women often do not gain from micro-credit. She effectively models the bargaining power as a function of the values of outside (or fallback) options. As long as the latter are much worse for women than for men, the threat of separation enables the man to claim the full benefit of the micro-credit. Translated to our setting, this might explain the transition rate to work for men to be less sensitive to a sanction for the woman than vice versa. Here it may be relevant to mention that in the case of a separation during the period of the benefits reduction, only the person who triggered the sanction will continue

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[7]The paper does not focus on effects on accepted wages, for the reason that the econometric identification of such effects with our data requires strong ad hoc functional form assumptions on aspects of the empirical model. Moreover, observations of starting wages in employment are sometimes missing or imprecise. In fact, welfare recipients often have a rather low productivity level, and their accepted wages are close to (or at) the wage floor that we set in the market, so any variation in wage offers will be small. However, a careful assessment of long-run effects of monitoring and sanctions on individual expected present values should take long-run effects on earnings into account.

to face the benefits cut.

We now consider couples with childcare obligations. First of all, recall the institutional feature that in households with a child aged below 3, one of the partners is not obliged to search for work. The relevant individual (usually the woman) is therefore expected to have a low transition rate to work and a low sanction rate. Now consider couples with children aged 3 and above where at most one of the partners can work, because one adult needs to be at home full-time to take care of the child(ren). We assume that the household decides in advance which of the two partners fulfills this role, based on the labor market prospects $(\lambda_i, w_i)$ of each of them. The caring partner will also face high unit search costs while unemployed. It is plausible that the home-making individual (say, $i = 1$) will exert effort $s_1 = 0$ whereas the other may have the highest possible value of $s_2$. This could be a value substantially higher than the threshold $s^*$ used by the monitoring agency. In this setting, a sanction triggered by $i = 1$ will not affect $s_2$ and therefore the transition rate to employment of $i = 2$ will not be affected. Conversely, $i = 1$ will be obliged to increase $s_1$ up to $s^*$, so its transition rate to work will increase. This scenario again assumes a stark asymmetry between the partners and depends on the existence of some physical upper bound on effort. The latter leads one of the partners to adopt a "corner solution" (i.e., the effort at the boundary of the range of possible values). More in general, corner solutions (and the resulting insensitivity to events occurring over time) may reflect physical or institutional constraints on behavior.

Finally, in a cooperative model setting, the joint expected present value of the household is maximized, where search costs are borne by both partners. A sanction can not lead to an increase in this joint value. To study effects of sanctions on transition rates to employment in detail, it is essential to be specific about the joint search cost function and about how efforts translate into job offer arrival rates of the partners. More in general, a cost function that is more flexible than in Equation (1) may cause model properties to depend strongly on complementarity of substitutability characteristics of the function. A detailed analysis is beyond the scope of the paper.

# 4 Data, sample selection and descriptives

The analysis is based on administrative data from two databases: the Integrated Employment Biographies and the Unemployment Benefit II Recipient History. The former contain information on spells of contributory and marginal part-time employment, registered unemployment, job search, and participation in ALMP. The Unemployment Benefit II Recipient History contains spell data on periods of UB II receipt and a household identifier. The latter allows us to identify couples within the same household.

Our sample consists of welfare recipients who are in a partner relationship in the same household and who both begin periods of welfare receipt while not being employed, on the same date between April 2008 and March 2013. We focus on the effect of the first strong sanction of one of the partners. We only include heterosexual couples in our analysis because the number of homosexual couples in the data is too low for a separate analysis. Hence, each couple consists of a female and a male partner. We focus on couples where both partners are aged between 25 and 55. We further restrict the sample by excluding couples where one of the partners received a sanction on the first day of their spell. Due to the administrative processes described in section 2, it takes several weeks from the infringement until the caseworker imposes a sanction. Thus, sanctions that are received on the first day of the spell are likely the result of an infringement that took place before the spell under observation. We exclude a minor share of observations with missing values in relevant variables.

To ensure that the welfare recipients in the sample are available for labor market integration and work (and thus have to comply with the requirements described in section 2), we require the partners to be non-employed when entering welfare. This means that they are not in contributory employment, in longer training measures, or in public-sponsored self-employment. They may however participate in other ALMP or work in marginal part-time employment. We refer to any uninterrupted period in which a person receives welfare benefits and is non-employed as *"a spell"* of that person. In addition, we restrict the sample to couples where both partners are registered as job seekers at the beginning of the spell.

A spell of a couple either ends with a transition to employment of one or both of the

partners or is right-censored. Transitions to employment are defined to occur if the spell ends with uptake of contributory employment within 93 days after the spell ends. We only consider a transition to employment if it involves monthly earnings above €400 and the job lasts longer than seven days. Hence, we do not consider transitions to marginal part-time employment or to jobs that are only short-term or with an income that may not suffice to overcome welfare receipt. If no transition to employment is observed, spells are censored when welfare receipt or non-employment ends, e.g., because the person enters a longer ALMP or the couple separates.[8] The observation window ends on December 31, 2015.

When the spell of one of the partners ends due to employment uptake or censoring, we right-censor the spell of their partner. The right-censoring of an individual's spell in case of the employment uptake of the partner is not independent right-censoring. However, the dependence is properly taken into account in the maximum likelihood estimation because the model includes the dynamic process for each partner and allows for related unobserved heterogeneity across those processes (see the next section).

When a couple leaves and re-enters the welfare system during the observation window, we include their spells in our analysis and treat them as being independent of one another conditional on observed and unobserved covariates. Our data includes 315,537 unique couples and 356,510 couple spells, which means that around 11% of all couple spells in our sample are re-entries of a couple into the welfare system.[9]

Table 1 displays summary statistics of the sample. Men in the sample are on average 41 years old and women are slightly younger, with an average of 38 years. About 42% of men and 34% of women have obtained a vocational degree, but only a minor share has obtained a university degree.

[Table 1 about here]

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[8]Van den Berg et al. (2022) document—next to a positive impact on the probability of finding a job—an increased probability of dropping out of the labor force for *young* sanctioned welfare recipients living in single households. The probability of dropping out of the labor force is not affected for sanctioned welfare recipients living in multi-person households.

[9]In the observation window, couple households may have several periods of welfare receipt. Each of these periods of welfare receipt may contain several periods during which one of the partners or both partners are non-employed. In each of a couple household's periods of welfare receipt, we focus only on the first period during which both partners are non-employed. When the non-employment spell of one of the partners ends due to a change in her/his status, we right-censor the other partner's spell at this point in time, as described in the previous paragraph.

Men are more often sanctioned than women in our sample (5.6% compared to 3%). Table 2 shows the number of couple spells of whom no, one, or both partners are sanctioned. If one of the partners receives a first strong sanction after their partner was already sanctioned but before one of them finds employment or the couple spell is censored, we observe two sanctions for the couple. This is the case for 1.1% of couple spells in our sample. Note that these sanctions need not to occur at the same time. The share of welfare recipients who make a transition to employment is higher for men with 45% compared to women with 16%.

[Table 2 about here]

Figure 1 plots life table estimates of the monthly empirical conditional transition probabilities (or "rates") into the first sanction and into employment. In panel (a), the transition rate to the first strong sanction reflects well-known patterns that have been analyzed by, e.g., Wolff and Moczall (2012): the transition rates peak shortly after the beginning of the spell and then gradually decline. Reasons for this pattern may be that caseworkers propose participation in ALMP and job offers more often earlier than later during the spell and, therefore, there is more scope to apply sanctions. An explanation for higher sanction rates for men might be that women receive fewer job offers or ALMP participation offers by the job center, partially because they are often the primary caretakers of children (Knize, 2022).

The transition rate into employment in panel (b) is highest shortly after the beginning of the spell and subsequently declines over the spell duration. This may reflect sorting over the duration of the spell, with welfare recipients with lower employment chances remaining unemployed for a longer time.

[Figure 1 about here]

# 5 Empirical model

## 5.1 Hazard rates

We are interested in the impact of the first sanction on the transition rates from welfare to employment of the sanctioned benefit recipient and their partner. For each couple spell,

we may distinguish between four duration variables: the duration from welfare until own employment of the man and of the woman, and the duration until a welfare sanction of the man and the woman. As pointed out in section 3, the four corresponding hazard rates and hence the durations themselves may be jointly influenced by stochastically related unobserved determinants. The data do not contain instrumental variables to enable causal inference in the presence of related unobserved confounders across partners. We therefore apply the "Timing of Events" approach (Abbring and van den Berg, 2003) and estimate a multidimensional mixed proportional hazard rates models.

The estimation is based on an inflow sample of couples into welfare receipt and unemployment. Each couple includes a female ($f$) and a male ($m$) partner. We model the transition rates from welfare to employment $\theta_{ef}(t)$ and $\theta_{em}(t)$ for both partners. We assume that all individual differences in these transition rates can be characterized by observed characteristics $x_f(t)$ and $x_m(t)$, systematic unobserved characteristics $V_{ef}$ and $V_{em}$, and the impacts of the own sanction and the partner's sanction. Similarly, the transition rates to sanctions $\theta_{sf}(t)$ and $\theta_{sm}(t)$ depend on observed characteristics $x_f(t)$ and $x_m(t)$ and systematic unobserved characteristics $V_{sf}$ and $V_{sm}$. We additionally allow the partner's sanction to affect the hazard rate of the duration until the own sanction.

We specify the conditional transition rates as piecewise constant in the elapsed duration, allowing for flexible duration dependencies. The transition rates to sanctions can be expressed as

$$
\begin{aligned}
\theta_{sf}(t) &= \exp\Big( \sum_{j=2}^{J} I_j(t)\lambda_{jsf} + x_f(t)'\beta_{sf} + I_{sm}(t > t_{sm})\alpha_{sf} + V_{sf} \Big) \\
\theta_{sm}(t) &= \exp\Big( \sum_{j=2}^{J} I_j(t)\lambda_{jsm} + x_m(t)'\beta_{sm} + I_{sf}(t > t_{sf})\alpha_{sm} + V_{sm} \Big).
\end{aligned}
\tag{2}
$$

and the transition rates from welfare to employment as

$$
\begin{aligned}
\theta_{ef}(t) &= \exp\Big( \sum_{j=2}^{J} I_j(t)\lambda_{jef} + x_f(t)'\beta_{ef} + I_{sf}(t > t_{sf})\alpha_{ef} + I_{sm}(t > t_{sm})\alpha_{efm} + V_{ef} \Big) \\
\theta_{em}(t) &= \exp\Big( \sum_{j=2}^{J} I_j(t)\lambda_{jem} + x_m(t)'\beta_{em} + I_{sm}(t > t_{sm})\alpha_{em} + I_{sf}(t > t_{sf})\alpha_{emf} + V_{em} \Big)
\end{aligned}
\tag{3}
$$

The indicator function $I_j(.)$ takes the value 1 if $t$ is in interval $j$. Parameters $\lambda_{jsm}$, $\lambda_{jsf}$, $\lambda_{jem}$, and $\lambda_{jef}$ describe the interval-specific baseline hazard levels. The variables $t_{sm}$ and $t_{sf}$ indicate the first day of the sanction for the male and the female welfare recipient, respectively, while $\alpha_{em}$ and $\alpha_{ef}$ are the effects of the first own sanction on the transition rate from welfare to employment for male and female job-seekers, and $\alpha_{emf}$ and $\alpha_{efm}$ capture the effects of the first sanction of the female and male partner. The effects of the partner's sanction on the transition rate to the own sanction correspond to $\alpha_{sm}$ and $\alpha_{sf}$. $x_f(t)$ and $x_m(t)$ are control variables for socio-demographics (age, education, nationality, and disability status), the household composition (number of children and whether children in different age groups live in the household), labor market characteristics (federal state, local unemployment rate, local long-term unemployment rate,[10] and local vacancy-to-unemployment ratio), and year and quarter of inflow. We additionally control for partner's age, education, nationality, and disability status. Household composition and labor market characteristic variables are time-varying and allowed to change every three calendar months.

## 5.2 Identification

This subsection briefly summarizes the intuition behind model identification and the validity of the key underlying assumption. Formal model identification follows straightforwardly from Abbring and van den Berg (2003) (see also Crépon et al. (2018) for an example of formal identification in a similar model framework in a fully different application). Identification of the causal sanction effects is driven by the relative timing of the sanctions. Intuitively, selection effects based on unobserved confounders create a global statistical dependence between cause and effect that is present at all durations, while the causal effects of own and partner's sanction create a local dependence, as they only have an effect from the moment at which the sanction is imposed onwards. Identification is based on the assumption that sanctions do not affect transition rates before the moment of their implementation. This is referred to as the "no anticipation" assumption. Anticipation of the own or the

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[10]We define as the local unemployment rate the number of unemployed relative to the dependent civilian labor force in the district and the local long-term unemployment rate as the number of long-term unemployed (unemployment duration of one year or more) relative to the dependent civilian labor force in the district.

partner's sanction that is not captured by observed or unobserved covariates would bias effect estimates, especially when individuals take up employment before the benefit reduction took place. Then we would not observe the treatment and we would attribute these effects to the non-treated group. This would probably lead to an underestimation of the effect if we assume that the true effects of the own and the partner's sanction on the transition rate to employment are positive.

The "no anticipation" assumption cannot be tested empirically.[11] We argue that anticipation is not relevant in our context and provide external evidence from caseworker interviews that speak to the assumption's plausibility. First, welfare recipients cannot be entirely sure if an infringement will lead to a sanction. There exist various reasons why a sanction may not be implemented even if an infringement took place, as described in section 2. For example, some caseworkers state that if a welfare recipient did not apply for a vacancy referral provided by the caseworker, the infringement is only detected if the employer informs the caseworker or if the caseworker find the time to inquire about the application with the employer. The process is similar if welfare recipients do not participate or abort participation in an ALMP. In addition, a sanction is not imposed if a "good justification" is accepted by the caseworker. We do not observe this in the data, but the caseworker interviews suggest that the rate of not imposed sanctions after a detected infringement ranges from 10% to 50%.

Furthermore, though welfare recipients may receive a notification before the imposition of their own sanction, it is not very likely that these notifications will lead to a transition to employment before the actual reduction in benefits. The duration between the first and the second notification is usually about three weeks, according to information provided by caseworker interviews. The median time between the second notification and the actual sanction date is 17 to 18 days (van den Berg et al., 2022). Given that welfare recipients are often long-term unemployed and/or low-skilled, the durations between the notifications and the beginning of the benefit reduction are probably not sufficient for the vast majority of welfare recipients to find employment. For anticipation of the partner sanction, the same

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[11]Data on job search effort throughout the welfare spell could help to obtain additional insights in this respect. Such information is however not available.

arguments apply. It is even less likely that anticipation matters in the case of the partner sanction because the information that a person was sanctioned may not always be shared between partners in the same household.

During the qualitative interviews with caseworkers, we explicitly asked about the share of welfare recipients that leave welfare receipt or take up employment because of one of the notifications before the benefit reduction comes into force. Several caseworkers could not answer this question because they could not recall that this ever happened, or it concerned only a very few cases. Among the few caseworkers who reported a share, this share ranges between 1% and 5%. Some caseworkers emphasized that it is not the notifications but the reduction of the benefit that matters and that they see sanction effects—if at all—only after the benefit reduction.

## 5.3 Distribution of unobserved heterogeneity

The unobserved heterogeneity terms in the four hazard rates, $V_{sf}$, $V_{sm}$, $V_{ef}$, and $V_{em}$, are assumed to be time-constant, to be statistically independent of observed covariates, and to satisfy possibly related discrete distributions with $p = 1, \dots, P$ support points or classes. Specifically, we assume a single underlying discrete distribution and add factor loadings that are specific for each of the four hazard rates. We parameterize the class probabilities associated with each support point using a multinomial logit ensuring that the probabilities range between zero and one and sum to one:

$$\pi_p = \frac{exp(\omega_p)}{\sum_{p=1}^{P} exp(\omega_p)}, \quad p = 1, ..., P, \quad \omega_1 = 0$$

With the functional form with factor loadings, the covariance matrix of the unobserved components is specified flexibly, and the number of parameters increases rapidly with $P$. In the estimation routine we increase the number of support points until the model fit cannot be further improved. Here, we rely on the Akaike Information Criterion to evaluate the model fit.[12]

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[12]Gaure et al. (2007) provide Monte Carlo evidence that modelling selection based on unobservables by way of a flexible discrete distribution works well in the context of "Timing of Events" models. Furthermore,

The model is estimated with Maximum Likelihood. For a given couple and for given values of the unobserved heterogeneity terms, the likelihood contribution is the product of a number of duration densities, possibly involving right-censoring. Summing these over all the possible values of the unobserved heterogeneity terms, weighing them with the corresponding probabilities, then gives the unconditional likelihood contribution of the couple. The likelihood is maximized over all unknown parameters including all parameters of the unobserved heterogeneity distribution. This procedure is rather straightforward and has been applied in a large number of empirical studies in which multiple durations and treatments for a given unit are combined; see Bonnal et al. (1997) and Crépon et al. (2018) for examples in the setting of ALMP evaluations.

# 6 Results

We start by estimating a model with sanction effects that do not vary with the time elapsed since imposition. After that, we allow these to be different in the first three months after imposition than in the period thereafter. Finally, we investigate effect heterogeneity with respect to the household composition.

In the empirical specification, we control for the duration dependence and additional control variables as explained before. Based on the Akaike Information Criterion, we select the model with six support points, leading to 25 additional parameters compared with the model without unobserved heterogeneity. The full set of estimates with and without unobserved heterogeneity is in Appendix tables A1 and A2.

## 6.1 Baseline results

Table 3 displays the baseline results for the transition to employment. The own sanction has a significant positive effect on the transition rate to employment for both partners. For men, the coefficient is 0.31, which corresponds to a relative increase in the transition rate by approximately 36%. For women, these numbers are 0.28 and 32%, respectively. These findings are in line with the theoretical analysis above and with findings reported in earlier

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see Lombardi et al. (2024) for a justification of the Akaike Information Criterion in multivariate duration models with discrete unobserved heterogeneity.

studies on welfare recipients that did not take partners into account (van den Berg et al., 2004; van den Berg et al., 2022). Compared to those previous findings, the effect sizes are somewhat more modest. The differences compared to the study of young German welfare recipients by van den Berg et al. (2022) are not surprising, as the benefit reductions after a first sanction were much higher for youths than for the age group we analyze.

A sanction triggered by the male has a positive and significant effect on the transition rate to employment of the female. The coefficient equals 0.2, which corresponds to a relative increase in the transition rate by 23%. Conversely, males are not affected by a sanction triggered by their female partner, in the sense that the estimate is positive but insignificantly different from zero.

The estimation routine also provides coefficients on effects of the partner sanction on the transition rate to the own sanction. Here, and for all model varieties we estimate in the paper, a partner sanction leads to a strong reduction in the hazard rate to own sanctions. This empirical result is in accordance to the institutional and theoretical considerations in sections 2 and 3, respectively. Table 4 presents findings for the baseline model. The estimated effects are similar across gender (reductions of 78% and 81%).

The finding that males are not (or barely) affected by sanctions triggered by their female partners appears intriguing. As alluded to in section 3, one may consider a multitude of factors in understanding this. First, since $\theta_{em}$ does increase upon a sanction for the male himself, it follows that the male's effort before a sanction was smaller than $s^*$. This in turn suggests that intensified monitoring after a sanction for the female should likewise have increased the level of $\theta_{em}$ upon that sanction. The fact that it doesn't implies that the male's effort does not increase upon a sanction for the female, and hence that there should be a substantial risk of a subsequent additional sanction. However, the estimated sanction rate strongly decreases upon a sanction for the partner. Perhaps males are much less intensively monitored after a sanction for their partner whereas females are more intensively monitored after a sanction for the male. As we have seen, the baseline sanction rate for women is much lower than for men, so a gender-specific change in monitoring might reflect some sort of updating of the baseline gender-specific susceptibility.

One may conjecture that the lack of adjustment by the male to the sanction for his partner is more outspoken in certain situations. In particular, this applies to situations where the positively signed effect of the benefits reduction on $\theta_{em}$ is compensated by the (negative) disincentive effect of the knowledge that the female partner will move to work more quickly and thereby will bring in wage income. Another potentially relevant situation is when the male has much more bargaining power and hence is able to shift the full burden of his partner's sanction to the partner.

In the absence of additional information, the above deliberations remain somewhat speculative. Moreover, the estimates are biased if the baseline model specification is too restrictive. This is why in subsequent subsections we consider more general specifications.

[Table 3 about here]

[Table 4 about here]

## 6.2 Simulation

To obtain a sense of the size of sanction effects in terms of number of days out of work, we use the estimated model and the data to simulate median duration outcomes as a function of the counterfactual durations $t_{sm}$ and $t_{sf}$ at which sanctions are imposed. As point of departure, for men, we may compute the duration distribution corresponding to the estimated hazard rate $\theta_{em}(t)$, for given $x_m(t), t_{sm}, t_{sf}$ and $V_{em}$. Here, the determinants $x_m(t)$ and $V_{em}$ are taken as averages in the data and the estimated model, respectively. For women, the same procedure can be adopted, reversing the indices $m$ and $f$.

Notice that such a procedure captures direct effects of the sanctions on the duration out of work, which act from the moment of sanction imposition onwards. As indicated in previous sections, there are also indirect effects, occurring with a delay which to some extent is random. To see this more pointedly, consider again a male receiving a sanction. This sanction also affects the female exit rate to work, which speeds up her transition into work. If she finds a job before the male then her job acceptance usually leads to immediate loss of welfare and this will in turn affect the male's exit rate to work from that moment onwards. The last leg of this indirect effect on the male's duration out of work takes place outside

of the welfare system and is not estimated in our empirical analysis (accordingly, a spell out of work is right-censored upon job acceptance of the partner). A likewise sequence of events can take place after the female receives a sanction. From section 3 we conjecture that the indirect effect, when it starts to act, tends to reduce the exit rate to work as compared to when the partner has not yet accepted a job. Thus, the simulation approach sketched above may somewhat exaggerate (i.e., over-estimate in absolute terms) the negative effects of a sanction on the total number of days out of work. Similar issues are related to the occurrence of follow-up sanctions in the household.

The simulation results are displayed in Table 5. These underline that sanction effects on employment transitions translate into sizeable reductions of welfare and non-employment duration. Without a sanction, the man's median duration is 558 days. If the man is sanctioned at the beginning of their spell, the median duration reduces to 360 days (35% relative to the no-sanction scenario). If the sanction occurs after 6, 12, or 24 months, the reduction in median duration is less pronounced, as the probability to find work before the sanction increases. As follows directly from the point estimates in subsection 6.1, if the female partner is sanctioned, there is no notable change in the man's median duration.

The median duration of a woman without sanctions is 2,129 days, and this reduces to 1,474 days (i.e., by 31%) if she is sanctioned at the beginning of the spell. If the male partner is sanctioned at the beginning of his spell, her median duration reduces to 1,630 days (23%).

[Table 5 about here]

## 6.3 Time-varying sanction effects

We allow the sanction effect to differ between the three-month period in which the benefit reduction is in place and the period thereafter. This more flexible specification goes along with a statistically significant increase of the log-likelihood. Table 6 displays the results for the transition to employment. For both men and women, the effect of the own sanction is more pronounced during the benefit reduction period, but it remains positive and significant after the sanction period ends. For women, the partner sanction has a similar effect during the benefit reduction and thereafter. For men, we find a positive and significant effect of the

partner sanction during the benefit reduction. This result is more in line with the theoretical framework than the corresponding result for the baseline model of the previous subsection. However, after the benefit reduction, the effect is not statistically significant from zero, like in the baseline model.

[Table 6 about here]

Positive effects beyond the period of the benefit reduction reflect an increased effort of the job seekers to comply with the job search requirements. This increased effort can be due to an increased monitoring intensity by the caseworker after the first sanction and/or be due to updated beliefs about the sanction and monitoring scheme.

Table 7 displays the effects of partner sanctions on transition rates to own sanctions. For both men and women, the negative effect after 90 days is stronger than in the first 90 days. This may to some extent reflect a delay in the sequence from the first sanction to the ensuing behavioral change.

[Table 7 about here]

## 6.4   Effect heterogeneity

We investigate whether the effects of sanctions depend on whether the household includes children and the ages of such children.[13] The results for the effects on transitions to employment are reported in Table 8. In the absence of a sanction, men with children have a significantly higher transition rate to work than men without children. This child effect is more pronounced in the presence of small (aged 0 to 2) children.

The female transition rate to work is significantly lower if there are small children. This reflects the need to take care of small children and the institutional feature that women with small children tend not to face monitoring or sanctions. The latter is also relevant for understanding the other estimates in this subsection. The results in Table 9 show that

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[13]For ease of exposition, we focus on the model specification that does not include time-varying sanction effects. Results for a specification including time-varying sanction effects are similar, see Tables A3 and A4 in the Appendix. We point out that the estimated additive effects of other covariates related to child age, notably those counting the number of children aged below 15, do not noticeably change when allowing for interaction effects in this subsection.

women with small children almost never get a sanction. If a sanction arrives, such women reduce the transition rate to work, possibly because from then on the caretaking role is exchanged between man and woman. However, this finding is based on a small number of sanctions, and, correspondingly, the net effect is insignificant (the standard error is an order of magnitude larger than any other standard error in Table 8).

Regarding partner sanctions, we see, first of all, that they are rare among households with small children. In such households, their effects on the transition rates to employment are negative, most strongly for women. This again suggests a shift of childcare responsibilities from the sanctioned person to the non-sanctioned person. The pattern is different when children are older. We now see significantly positive effects of partner sanctions for each gender. Thus, the heterogeneity analysis by children's age composition *(i)* highlights the relevance of the less stringent monitoring of parents of young children, and *(ii)* makes the results for parents of older children being more in line with the theoretical framework, as compared to the homogeneous effects analysis.

[Table 8 about here]

Finally, Table 9 shows that a partner sanction unambiguously decreases the transition rate to an own sanction.

[Table 9 about here]

## 6.5   Robustness to sample selection

Our baseline sample consists of households entering welfare benefits between April 2008 and March 2013. As a robustness check, we restrict the sample to spells in which neither partner received welfare benefits in the year prior to the observed spell. Hence, we restrict the sample to couples who spent less time on welfare in the past and in turn may have less prior knowledge about the welfare system's institutional details regarding sanctions, so that for them potential anticipatory effects of sanctions may be smaller. This exercise thus sheds some light on the credibility of the no-anticipation assumption. We obtain 155,723 couple spells, representing less than half of the couple spells included in the main analysis. Based on the AIC, we again choose the model with six support points.

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The pattern of results is robust to this alternative sample setting. As Table 10 shows, the effects of both own and partner sanctions on the transition rate to employment are slightly more pronounced. In contrast to the main sample, we find a significantly positive effect of the partner sanction for men also in the period after the sanction is in force. The coefficient of the partner sanction effect for women in the period the sanction is in force is of similar magnitude compared to the main sample, but not statistically significant. The fact that the estimated effects in this robustness check are larger compared to our main results is consistent with a possible underestimation of the true effect due to attenuation bias resulting from anticipation, as discussed in section 5. For the effect of partner sanctions on the transition rate to individual sanctions in Table 11, we find results very similar to those in the baseline setting.[14]

[Table 10 about here]

[Table 11 about here]

# 7    Conclusion

Means-tested welfare benefits are provided at the household level. This paper demonstrates that, in two-partner households, a sanction for not complying with job search guidelines does not only affect the sanctioned individual but also the partner. Here it is crucial to distinguish between households with small children and households without them, because in the former, one of the adults is by default not subject to monitoring and sanctions. Indeed, in such households, the sanction rate for women is virtually zero, whereas in other household types it is not. Moreover, with small children, effects of partner sanctions on the own transition rate to employment tend to be negative, possibly reflecting a shift of childcare responsibilities from the sanctioned person to the non-sanctioned person.

Turning to the more common types of households, with older or no children, we find that partner sanctions increase the own transition rate to work. Interestingly, the magnitude of

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[14]The baseline results are based on a sample with repeated spells for part of the households. As an additional robustness check, we keep only the first spell of each couple during our observation period. This robustness check includes 315,537 of the 356,510 initial couple spells. The results are reported in Tables A5 and A6 in the Appendix. The estimated coefficients are similar to those in the baseline setting.

this effect is larger among women than among men. In other words, the effect of a female sanction on the male's employment is smaller than the effect of a male sanction on the female's employment. The paper discusses a number of explanations for this. First, the size of the partner sanction effect depends on the own and the partner's labor market conditions and prospects, and these may differ by gender. If the female receives a sanction, then she will move to work sooner and thereby bring in wage income sooner. This creates a disincentive for the man to search himself upon the female's sanction, which may compensate for the incentive due to the instantaneous reduction in total household benefits, especially if the man's search costs are high. Alternatively, it is possible that men are less intensively monitored after a sanction for their partner, whereas women are more intensively monitored after a sanction for the man. The baseline sanction rate for women is much lower than for men, so a gender-specific change in monitoring after a partner sanction might reflect some sort of updating of the baseline gender-specific susceptibility. A third explanation is that the male has more bargaining power and hence is able to assign the burden of his partner's sanction to the partner, whereas this is more difficult for the female. In the end, the "labor market prospects" explanation and the "bargaining power" explanation are intertwined, because the outside option of the woman is determined to a large extent by her labor market prospects.

The results confirm that sanctions are relatively effective in bringing welfare recipients to employment. However, the results also show that the partner of the sanctioned individual is affected. The effect on the partner is particularly pronounced in the presence of older children in the household. This raises questions about the extent to which such children are negatively affected and whether that is justifiable from a social equity point of view. It may be a relevant topic for further research to investigate whether the children suffer in terms of school performance and subsequent labor market outcomes.

The theoretical and empirical analyses draw attention to a more general issue. Notably, effects of monitoring and sanctions of the *partner* on an individual's transition rate to work can be profoundly different from the effects of monitoring and sanctions of the individual under consideration. As demonstrated above, this complicates the formulation of simple

explanations for the signs of the corresponding findings. Related to this is the fact that the connection between intra-household bargaining power on the one hand and the effect of a sanction on both partners' employment outcomes on the other hand, is indirect, complex, and fundamentally dependent on the individual labor market conditions of the two partners. Therefore the estimated sanction effects do not translate straightforwardly into statements about the relative bargaining power of the partners.

Clearly, these considerations have wider relevance and extend to the economic and econometric analysis of legal systems in which groups of individuals are collectively punished for violations by a single member of the group.

We identify a number of additional topics for further research. First, treatment effect sizes may not only vary with the elapsed time since the imposition of the sanction and with the household composition but also with the degree of employability of the two partners and even on unobservables. It would therefore be interesting to quantify the labor market prospects of the partners and incorporate these as effect size determinants. Unfortunately, with our data, it is not straightforward to pursue this idea given the often unsteady employment histories of welfare recipients in preceding years. Second, reduced-form analyses have limitations when studying the effects of counterfactual policy designs. For such purposes, a structural analysis would be appealing. However, to pursue this without running the risk that results are fully driven by functional-form assumptions, this would require observability of search effort of each partner.
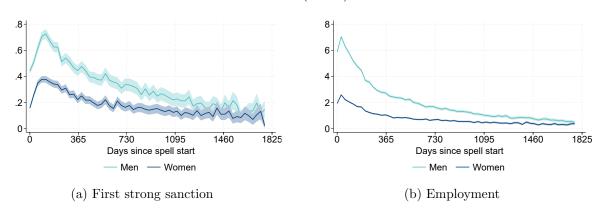
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# Tables and Figures

FIGURE 1

TRANSITION RATES INTO THE FIRST STRONG SANCTION AND INTO EMPLOYMENT PER
MONTH (IN %)



(a) First strong sanction

(b) Employment

Life table estimates of empirical transition rates and 95 % confidence intervals.
*Notes:* The estimation includes 356,510 spells for each men and women. A transition to employment is
defined as uptake of contributory employment with monthly earnings above €400 that last lasts at least
eight days within 93 days after the spell ends. Spells are censored, the latest on 31/12/2015.

Table 1: Mean of selected covariates at spell begin by gender

| | Men | Women |
|---|---|---|
| Age | 40.512 | 37.850 |
| No vocational degree | 0.446 | 0.456 |
| Vocational degree | 0.415 | 0.339 |
| University degree | 0.036 | 0.035 |
| Vocational degree missing | 0.104 | 0.169 |
| No schooling degree | 0.195 | 0.182 |
| Lower secondary degree (Hauptschule) | 0.421 | 0.309 |
| Secondary degree (Mittlere Reife) | 0.186 | 0.213 |
| Higher secondary degree | 0.081 | 0.076 |
| Schooling degree missing | 0.117 | 0.219 |
| German nationality | 0.710 | 0.702 |
| Turkish nationality | 0.102 | 0.088 |
| Severe disability | 0.032 | 0.020 |
| Any children aged 0 to 2 in household | 0.078 | 0.078 |
| Any children aged 3 to 5 in household | 0.209 | 0.209 |
| Any children aged 6 to 17 in household | 0.495 | 0.495 |
| Children under age 15 in household: none | 0.443 | 0.443 |
| Children under age 15 in household: one | 0.263 | 0.263 |
| Children under age 15 in household: two | 0.200 | 0.200 |
| Children under age 15 in household: three or more | 0.094 | 0.094 |
| Schleswig Holstein | 0.036 | 0.036 |
| Hamburg | 0.020 | 0.020 |
| Lower Saxony | 0.099 | 0.099 |
| Bremen | 0.011 | 0.011 |
| North Rhine-Westphalia | 0.270 | 0.270 |
| Hesse | 0.067 | 0.067 |
| Rhineland-Palatinate | 0.049 | 0.049 |
| Baden-Wuerttemberg | 0.091 | 0.091 |
| Bavaria | 0.095 | 0.095 |
| Saarland | 0.014 | 0.014 |
| Berlin | 0.055 | 0.055 |
| Brandenburg | 0.037 | 0.037 |
| Mecklenburg Western Pomerania | 0.031 | 0.031 |
| Saxony | 0.054 | 0.054 |
| Saxony-Anhalt | 0.041 | 0.041 |
| Thuringia | 0.030 | 0.030 |
| Entry year: 2008 | 0.148 | 0.148 |
| Entry year: 2009 | 0.253 | 0.253 |
| Entry year: 2010 | 0.221 | 0.221 |
| Entry year: 2011 | 0.170 | 0.170 |
| Entry year: 2012 | 0.160 | 0.160 |
| Entry year: 2013 | 0.048 | 0.048 |
| Entry quarter: first | 0.300 | 0.300 |
| Entry quarter: second | 0.229 | 0.229 |
| Entry quarter: third | 0.229 | 0.229 |
| Entry quarter: fourth | 0.242 | 0.242 |
| District unemployment rate | 9.721 | 9.721 |
| District long-term unemployment rate | 3.571 | 3.571 |
| District vacancy-unemployment ratio | 0.126 | 0.126 |
| Own strong sanction | 0.056 | 0.030 |
| Employment transition | 0.447 | 0.164 |
| Number of spells | 356,510 | 356,510 |

Table 2: Share of couples with no, male, female, or both partner's sanction

| | |
|---|---|
| None | 0.924 |
| Male | 0.046 |
| Female | 0.020 |
| Both | 0.011 |

*Notes:* inflow sample of welfare recipients living with their partner. The number of couple spells is 356,510.

Table 3: Own and partner sanction effects on the transition rate to employment

| | Coefficients and standard errors | |
|---|---|---|
| | Men | Women |
| Own sanction | 0.31 *** | 0.277 *** |
| | (0.037) | (0.063) |
| Partner sanction | 0.005 | 0.204 *** |
| | (0.038) | (0.057) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,468. Control variables are included. ***, **, *, indicate significance at the 1 %, 5 %, 10 % level

Table 4: Partner sanction effects on the transition rate to the own sanction

| | Coefficients and standard errors | |
|---|---|---|
| | Men | Women |
| Partner sanction | -1.523 *** | -1.678 *** |
| | (0.042) | (0.044) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,468. Control variables are included. ***, **, *, indicate significance at the 1 %, 5 %, 10 % level

Table 5: Median welfare and non-employment duration in days with and without sanction by gender

|  |  | Men | Women |
|---|---|---|---|
|  |  | 558 | 2,129 |
| No sanction |  | 558 | 2,129 |
| Own sanction after ... days | 0 | 360 | 1,474 |
|  | 180 | 443 | 1,565 |
|  | 360 | 503 | 1,639 |
|  | 720 | 558 | 1,763 |
| Partner sanction after ... days | 0 | 554 | 1,630 |
|  | 180 | 556 | 1,699 |
|  | 360 | 557 | 1,755 |
|  | 720 | 558 | 1,850 |

*Notes:* The table shows welfare recipients' median welfare and non-employment duration if they are not sanctioned and if they are sanctioned after 0, 180, 360, or 720 days. The median duration is calculated for men and women with average characteristics in terms of observables and unobservables and based on the estimates of own and partner sanction effects.

Table 6: Own and partner sanction effects on the transition rate to employment

|  | Coefficients and standard errors | |
|---|---|---|
|  | Men | Women |
| Own sanction (0-90 days) | 0.333 *** | 0.419 *** |
|  | (0.038) | (0.073) |
| Own sanction (90+ days) | 0.235 *** | 0.159 ** |
|  | (0.038) | (0.066) |
| Partner sanction (0-90 days) | 0.119 *** | 0.149 ** |
|  | (0.045) | (0.063) |
| Partner sanction (90+ days) | -0.078 * | 0.141 ** |
|  | (0.04) | (0.059) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,395. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table 7: Partner sanction effects on the transition rate to the own sanction

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Partner sanction (0-90 days) | -1.344 *** | -1.513 *** |
| | (0.056) | (0.057) |
| Partner sanction (90+ days) | -1.805 *** | -1.955 *** |
| | (0.049) | (0.052) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,395. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table 8: Own and partner sanction effects on the transition rate to employment: heterogeneous effects

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Any children aged 0 to 2 | 0.1 *** | -0.225 *** |
| | (0.011) | (0.021) |
| Any children aged 3 to 5 | 0.04 *** | -0.202 *** |
| | (0.009) | (0.017) |
| Any children aged 6 to 17 | 0.051 *** | 0.006 |
| | (0.009) | (0.016) |
| Own sanction | 0.229 *** | 0.253 *** |
| | (0.04) | (0.068) |
| Partner sanction | -0.069 | 0.112 * |
| | (0.045) | (0.061) |
| Own sanction x any children aged 0 to 2 in household | 0.014 | -0.56 *** |
| | (0.035) | (0.208) |
| Own sanction x any children aged 3 to 5 in household | 0.078 ** | -0.063 |
| | (0.032) | (0.103) |
| Own sanction x any children aged 6 to 17 in household | 0.102 *** | 0.06 |
| | (0.027) | (0.057) |
| Partner sanction x any children aged 0 to 2 in household | -0.146 * | -0.402 *** |
| | (0.085) | (0.088) |
| Partner sanction x any children aged 3 to 5 in household | -0.012 | 0.207 *** |
| | (0.055) | (0.067) |
| Partner sanction x any children aged 6 to 17 in household | 0.153 *** | 0.182 *** |
| | (0.039) | (0.047) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,402. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table 9: Own and partner sanction effects on the transition rate to the own sanction: heterogeneous effects

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Any children aged 0 to 2 | -0.065 * | -2.124 *** |
| | (0.033) | (0.091) |
| Any children aged 3 to 5 | -0.114 *** | -0.342 *** |
| | (0.028) | (0.042) |
| Any children aged 6 to 17 | 0.013 | 0.026 |
| | (0.03) | (0.041) |
| Partner sanction | -1.579 *** | -1.752 *** |
| | (0.053) | (0.055) |
| Partner sanction x any children aged 0 to 2 in household | -0.259 * | -0.295 |
| | (0.136) | (0.19) |
| Partner sanction x any children aged 3 to 5 in household | 0.216 ** | 0.17 ** |
| | (0.087) | (0.08) |
| Partner sanction x any children aged 6 to 17 in household | 0.026 | 0.033 |
| | (0.061) | (0.06) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,402. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table 10: Own and partner sanction effects on the transition rate to employment: no welfare receipt in the latest pre-welfare year

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Own sanction (0-90 days) | 0.657 *** | 0.719 *** |
| | (0.07) | (0.118) |
| Own sanction (90+ days) | 0.612 *** | 0.458 *** |
| | (0.073) | (0.112) |
| Partner sanction (0-90 days) | 0.39 *** | 0.169 |
| | (0.077) | (0.117) |
| Partner sanction (90+ days) | 0.177 ** | 0.369 *** |
| | (0.076) | (0.107) |

*Notes:* The estimation includes only those couple spells without any welfare benefit receipt in the year before inflow. Joint estimation for both partners (P = 6). The number of couple spells is 155,723 and the log-likelihood = -864,989. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table 11: Partner sanction effect on the transition rate to the own sanction: no welfare receipt in the latest pre-welfare year

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Partner sanction (0-90 days) | -1.354 *** | -1.684 *** |
| | (0.107) | (0.105) |
| Partner sanction (90+ days) | -2.103 *** | -2.166 *** |
| | (0.098) | (0.092) |

*Notes:* The estimation includes only those couple spells without any welfare benefit receipt in the year before inflow. Joint estimation for both partners (P = 6). The number of couple spells is 155,723 and the log-likelihood = -864,989. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

# Supplementary material

Table A1: Full model estimates without unobserved heterogeneity

| | Transition rate into own sanction | | | | Transition rate into employment | | | |
|---|---|---|---|---|---|---|---|---|
| | Beta | S.E. | Beta | S.E. | Beta | S.E. | Beta | S.E. |
| Constant | −7.343 | 0.217 | −8.894 | 0.296 | −5.805 | 0.076 | −8.832 | 0.126 |
| Months 4-6 | 0.313 | 0.021 | 0.402 | 0.030 | −0.244 | 0.007 | −0.243 | 0.012 |
| Months 7-9 | 0.147 | 0.024 | 0.275 | 0.034 | −0.551 | 0.009 | −0.536 | 0.014 |
| Months 10-12 | −0.003 | 0.028 | 0.127 | 0.038 | −0.789 | 0.010 | −0.713 | 0.017 |
| Months 13-15 | −0.100 | 0.031 | −0.055 | 0.044 | −0.934 | 0.012 | −0.901 | 0.020 |
| Months 16-18 | −0.268 | 0.036 | −0.182 | 0.050 | −1.009 | 0.014 | −0.948 | 0.022 |
| Months 19-24 | −0.297 | 0.031 | −0.246 | 0.043 | −1.200 | 0.012 | −1.113 | 0.020 |
| Months 25-36 | −0.531 | 0.030 | −0.449 | 0.042 | −1.491 | 0.012 | −1.293 | 0.019 |
| Months 37+ | −1.138 | 0.035 | −0.933 | 0.045 | −2.086 | 0.014 | −1.724 | 0.020 |
| Age | 0.022 | 0.010 | 0.061 | 0.014 | −0.001 | 0.004 | 0.092 | 0.006 |
| Age$^2$ | −0.001 | 0.000 | −0.001 | 0.000 | 0.000 | 0.000 | −0.001 | 0.000 |
| Partner: age | −0.003 | 0.010 | −0.007 | 0.013 | −0.007 | 0.004 | −0.004 | 0.006 |
| Partner: age$^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| German nationality | 0.170 | 0.022 | 0.275 | 0.031 | −0.041 | 0.008 | 0.277 | 0.013 |
| Turkish nationality | 0.089 | 0.036 | −0.045 | 0.055 | 0.020 | 0.013 | −0.136 | 0.027 |
| Partner: German nationality | 0.071 | 0.022 | 0.050 | 0.031 | 0.026 | 0.008 | −0.021 | 0.013 |
| Partner: Turkish nationality | 0.114 | 0.036 | 0.030 | 0.052 | 0.031 | 0.014 | −0.036 | 0.024 |
| Vocational degree | −0.127 | 0.017 | −0.208 | 0.025 | 0.244 | 0.006 | 0.416 | 0.011 |
| University degree | −0.505 | 0.068 | −0.742 | 0.103 | 0.104 | 0.017 | 0.413 | 0.024 |
| Vocational degree missing | −0.028 | 0.031 | −0.146 | 0.035 | −0.079 | 0.012 | 0.038 | 0.017 |
| Partner: vocational degree | −0.157 | 0.019 | −0.088 | 0.024 | 0.065 | 0.007 | 0.090 | 0.010 |
| Partner: university degree | −0.425 | 0.066 | −0.398 | 0.093 | 0.001 | 0.018 | 0.011 | 0.025 |
| Partner: vocational degree missing | −0.044 | 0.024 | 0.025 | 0.041 | 0.009 | 0.009 | 0.068 | 0.018 |
| No schooling degree | −0.039 | 0.018 | 0.031 | 0.026 | −0.145 | 0.007 | −0.276 | 0.014 |
| Secondary degree | −0.168 | 0.023 | −0.219 | 0.031 | 0.020 | 0.007 | 0.174 | 0.011 |
| Higher secondary degree | −0.354 | 0.039 | −0.387 | 0.059 | −0.098 | 0.012 | 0.296 | 0.017 |
| Schooling degree missing | −0.545 | 0.033 | −0.355 | 0.036 | −0.456 | 0.012 | −0.314 | 0.016 |
| Partner: no schooling degree | 0.046 | 0.019 | 0.017 | 0.025 | −0.080 | 0.008 | −0.092 | 0.013 |
| Partner: secondary degre | −0.221 | 0.023 | −0.176 | 0.032 | 0.067 | 0.007 | 0.085 | 0.012 |
| Partner: higher secondary degree | −0.421 | 0.041 | −0.289 | 0.055 | −0.012 | 0.012 | 0.078 | 0.018 |
| Partner: schooling degree missing | −0.199 | 0.025 | −0.243 | 0.043 | 0.034 | 0.009 | 0.023 | 0.018 |
| Severe disability | −1.099 | 0.064 | −1.220 | 0.114 | −0.621 | 0.018 | −0.526 | 0.033 |
| Partner: severe disability | −0.185 | 0.058 | −0.257 | 0.058 | −0.015 | 0.018 | 0.152 | 0.020 |
| Entry year: 2009 | −0.131 | 0.022 | −0.063 | 0.031 | 0.085 | 0.009 | 0.075 | 0.014 |
| Entry year: 2010 | −0.039 | 0.023 | 0.042 | 0.032 | 0.232 | 0.009 | 0.131 | 0.014 |
| Entry year: 2011 | −0.113 | 0.025 | 0.007 | 0.035 | 0.163 | 0.010 | 0.116 | 0.016 |
| Entry year: 2012 | −0.188 | 0.026 | −0.012 | 0.036 | 0.082 | 0.010 | 0.088 | 0.016 |
| Entry year: 2013 | −0.327 | 0.046 | −0.111 | 0.061 | 0.149 | 0.014 | 0.135 | 0.025 |
| Entry quarter: second | 0.011 | 0.021 | −0.019 | 0.029 | −0.130 | 0.007 | −0.048 | 0.012 |
| Entry quarter: third | 0.041 | 0.021 | 0.022 | 0.029 | −0.221 | 0.007 | −0.063 | 0.012 |
| Entry quarter: fourth | −0.022 | 0.021 | −0.037 | 0.029 | −0.191 | 0.007 | −0.072 | 0.012 |
| Children under age 15: one | −0.068 | 0.026 | −0.222 | 0.036 | 0.099 | 0.009 | −0.144 | 0.015 |
| Children under age 15: two | −0.091 | 0.033 | −0.331 | 0.044 | 0.121 | 0.012 | −0.307 | 0.020 |
| Children under age 15: three or more | 0.001 | 0.040 | −0.337 | 0.055 | 0.016 | 0.015 | −0.640 | 0.027 |
| Any children aged 0 to 2 | −0.020 | 0.026 | −2.095 | 0.077 | 0.107 | 0.010 | −0.227 | 0.019 |
| Any children aged 3 to 5 | −0.027 | 0.022 | −0.217 | 0.034 | 0.046 | 0.008 | −0.184 | 0.016 |
| Any children aged 6 to 17 | 0.018 | 0.024 | 0.019 | 0.034 | 0.061 | 0.008 | 0.022 | 0.014 |
| Schleswig Holstein | 0.018 | 0.043 | −0.063 | 0.060 | −0.025 | 0.015 | 0.224 | 0.023 |
| Hamburg | 0.176 | 0.051 | 0.217 | 0.069 | −0.219 | 0.021 | 0.058 | 0.031 |
| Lower Saxony | 0.022 | 0.028 | 0.019 | 0.039 | 0.030 | 0.010 | 0.059 | 0.017 |
| Bremen | −0.164 | 0.077 | −0.130 | 0.106 | −0.104 | 0.026 | 0.015 | 0.044 |
| Hesse | 0.025 | 0.031 | −0.099 | 0.046 | 0.055 | 0.011 | 0.102 | 0.019 |
| Rhineland-Palatinate | 0.159 | 0.035 | 0.186 | 0.048 | 0.100 | 0.013 | 0.020 | 0.023 |
| Baden-Wuerttemberg | 0.143 | 0.032 | 0.225 | 0.045 | 0.053 | 0.012 | 0.119 | 0.020 |
| Bavaria | 0.263 | 0.032 | 0.344 | 0.044 | 0.150 | 0.011 | 0.237 | 0.019 |
| Saarland | −0.124 | 0.067 | −0.318 | 0.098 | 0.043 | 0.022 | −0.098 | 0.041 |
| Berlin | 0.268 | 0.043 | 0.289 | 0.057 | −0.420 | 0.016 | 0.131 | 0.023 |
| Brandenburg | −0.020 | 0.050 | 0.029 | 0.065 | −0.127 | 0.015 | 0.092 | 0.023 |
| Mecklenburg Western Pomerania | 0.016 | 0.064 | 0.148 | 0.081 | −0.214 | 0.020 | 0.035 | 0.030 |
| Saxony | 0.067 | 0.041 | 0.175 | 0.054 | −0.152 | 0.013 | −0.022 | 0.021 |
| Saxony-Anhalt | 0.058 | 0.048 | 0.078 | 0.062 | −0.131 | 0.015 | −0.077 | 0.024 |
| Thuringia | 0.107 | 0.054 | 0.120 | 0.072 | −0.039 | 0.016 | 0.057 | 0.025 |
| District unemployment rate | −0.031 | 0.008 | −0.002 | 0.011 | 0.040 | 0.003 | 0.038 | 0.004 |
| District long-term unemployment rate | −0.044 | 0.015 | −0.090 | 0.020 | −0.109 | 0.005 | −0.127 | 0.008 |
| District vacancy-unemployment ratio | 0.605 | 0.110 | 0.079 | 0.160 | 0.713 | 0.041 | 0.752 | 0.069 |
| Partner sanction (0-90 days) | 1.159 | 0.050 | 1.028 | 0.052 | - | - | - | - |
| Partner sanction (90+ days) | 0.996 | 0.034 | 1.075 | 0.035 | - | - | - | - |
| Own sanction | - | - | - | - | 0.145 | 0.012 | 0.158 | 0.026 |
| Partner sanction | - | - | - | - | −0.078 | 0.018 | 0.001 | 0.022 |

*Notes:* First, second, fifth and sixth column refer to men; the others to women. The number of couple spells is 356,510 and the log-likelihood = -1,996,761.

Table A2: Full model estimates with unobserved heterogeneity

| | Transition rate into own sanction | | | | Transition rate into employment | | | |
|---|---|---|---|---|---|---|---|---|
| | Beta | S.E. | Beta | S.E. | Beta | S.E. | Beta | S.E. |
| Constant | −7.343 | 0.217 | −8.894 | 0.296 | −5.805 | 0.076 | −8.832 | 0.126 |
| Months 4-6 | 0.313 | 0.021 | 0.402 | 0.030 | −0.244 | 0.007 | −0.243 | 0.012 |
| Months 7-9 | 0.147 | 0.024 | 0.275 | 0.034 | −0.551 | 0.009 | −0.536 | 0.014 |
| Months 10-12 | −0.003 | 0.028 | 0.127 | 0.038 | −0.789 | 0.010 | −0.713 | 0.017 |
| Months 13-15 | −0.100 | 0.031 | −0.055 | 0.044 | −0.934 | 0.012 | −0.901 | 0.020 |
| Months 16-18 | −0.268 | 0.036 | −0.182 | 0.050 | −1.009 | 0.014 | −0.948 | 0.022 |
| Months 19-24 | −0.297 | 0.031 | −0.246 | 0.043 | −1.200 | 0.012 | −1.113 | 0.020 |
| Months 25-36 | −0.531 | 0.030 | −0.449 | 0.042 | −1.491 | 0.012 | −1.293 | 0.019 |
| Months 37+ | −1.138 | 0.035 | −0.933 | 0.045 | −2.086 | 0.014 | −1.724 | 0.020 |
| Age | 0.022 | 0.010 | 0.061 | 0.014 | −0.001 | 0.004 | 0.092 | 0.006 |
| Age$^2$ | −0.001 | 0.000 | −0.001 | 0.000 | 0.000 | 0.000 | −0.001 | 0.000 |
| Partner: age | −0.003 | 0.010 | −0.007 | 0.013 | −0.007 | 0.004 | −0.004 | 0.006 |
| Partner: age$^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| German nationality | 0.170 | 0.022 | 0.275 | 0.031 | −0.041 | 0.008 | 0.277 | 0.013 |
| Turkish nationality | 0.089 | 0.036 | −0.045 | 0.055 | 0.020 | 0.013 | −0.136 | 0.027 |
| Partner: German nationality | 0.071 | 0.022 | 0.050 | 0.031 | 0.026 | 0.008 | −0.021 | 0.013 |
| Partner: Turkish nationality | 0.114 | 0.036 | 0.030 | 0.052 | 0.031 | 0.014 | −0.036 | 0.024 |
| Vocational degree | −0.127 | 0.017 | −0.208 | 0.025 | 0.244 | 0.006 | 0.416 | 0.011 |
| University degree | −0.505 | 0.068 | −0.742 | 0.103 | 0.104 | 0.017 | 0.413 | 0.024 |
| Vocational degree missing | −0.028 | 0.031 | −0.146 | 0.035 | −0.079 | 0.012 | 0.038 | 0.017 |
| Partner: vocational degree | −0.157 | 0.019 | −0.088 | 0.024 | 0.065 | 0.007 | 0.090 | 0.010 |
| Partner: university degree | −0.425 | 0.066 | −0.398 | 0.093 | 0.001 | 0.018 | 0.011 | 0.025 |
| Partner: vocational degree missing | −0.044 | 0.024 | 0.025 | 0.041 | 0.009 | 0.009 | 0.068 | 0.018 |
| No schooling degree | −0.039 | 0.018 | 0.031 | 0.026 | −0.145 | 0.007 | −0.276 | 0.014 |
| Secondary degree | −0.168 | 0.023 | −0.219 | 0.031 | 0.020 | 0.007 | 0.174 | 0.011 |
| Higher secondary degree | −0.354 | 0.039 | −0.387 | 0.059 | −0.098 | 0.012 | 0.296 | 0.017 |
| Schooling degree missing | −0.545 | 0.033 | −0.355 | 0.036 | −0.456 | 0.012 | −0.314 | 0.016 |
| Partner: no schooling degree | 0.046 | 0.019 | 0.017 | 0.025 | −0.080 | 0.008 | −0.092 | 0.013 |
| Partner: secondary degre | −0.221 | 0.023 | −0.176 | 0.032 | 0.067 | 0.007 | 0.085 | 0.012 |
| Partner: higher secondary degree | −0.421 | 0.041 | −0.289 | 0.055 | −0.012 | 0.012 | 0.078 | 0.018 |
| Partner: schooling degree missing | −0.199 | 0.025 | −0.243 | 0.043 | 0.034 | 0.009 | 0.023 | 0.018 |
| Severe disability | −1.099 | 0.064 | −1.220 | 0.114 | −0.621 | 0.018 | −0.526 | 0.033 |
| Partner: severe disability | −0.185 | 0.058 | −0.257 | 0.058 | −0.015 | 0.018 | 0.152 | 0.020 |
| Entry year: 2009 | −0.131 | 0.022 | −0.063 | 0.031 | 0.085 | 0.009 | 0.075 | 0.014 |
| Entry year: 2010 | −0.039 | 0.023 | 0.042 | 0.032 | 0.232 | 0.009 | 0.131 | 0.014 |
| Entry year: 2011 | −0.113 | 0.025 | 0.007 | 0.035 | 0.163 | 0.010 | 0.116 | 0.016 |
| Entry year: 2012 | −0.188 | 0.026 | −0.012 | 0.036 | 0.082 | 0.010 | 0.088 | 0.016 |
| Entry year: 2013 | −0.327 | 0.046 | −0.111 | 0.061 | 0.149 | 0.014 | 0.135 | 0.025 |
| Entry quarter: second | 0.011 | 0.021 | −0.019 | 0.029 | −0.130 | 0.007 | −0.048 | 0.012 |
| Entry quarter: third | 0.041 | 0.021 | 0.022 | 0.029 | −0.221 | 0.007 | −0.063 | 0.012 |
| Entry quarter: fourth | −0.022 | 0.021 | −0.037 | 0.029 | −0.191 | 0.007 | −0.072 | 0.012 |
| Children under age 15: one | −0.068 | 0.026 | −0.222 | 0.036 | 0.099 | 0.009 | −0.144 | 0.015 |
| Children under age 15: two | −0.091 | 0.033 | −0.331 | 0.044 | 0.121 | 0.012 | −0.307 | 0.020 |
| Children under age 15: three or more | 0.001 | 0.040 | −0.337 | 0.055 | 0.016 | 0.015 | −0.640 | 0.027 |
| Any children aged 0 to 2 | −0.020 | 0.026 | −2.095 | 0.077 | 0.107 | 0.010 | −0.227 | 0.019 |
| Any children aged 3 to 5 | −0.027 | 0.022 | −0.217 | 0.034 | 0.046 | 0.008 | −0.184 | 0.016 |
| Any children aged 6 to 17 | 0.018 | 0.024 | 0.019 | 0.034 | 0.061 | 0.008 | 0.022 | 0.014 |
| Schleswig Holstein | 0.018 | 0.043 | −0.063 | 0.060 | −0.025 | 0.015 | 0.224 | 0.023 |
| Hamburg | 0.176 | 0.051 | 0.217 | 0.069 | −0.219 | 0.021 | 0.058 | 0.031 |
| Lower Saxony | 0.022 | 0.028 | 0.019 | 0.039 | 0.030 | 0.010 | 0.059 | 0.017 |
| Bremen | −0.164 | 0.077 | −0.130 | 0.106 | −0.104 | 0.026 | 0.015 | 0.044 |
| Hesse | 0.025 | 0.031 | −0.099 | 0.046 | 0.055 | 0.011 | 0.102 | 0.019 |
| Rhineland-Palatinate | 0.159 | 0.035 | 0.186 | 0.048 | 0.100 | 0.013 | 0.020 | 0.023 |
| Baden-Wuerttemberg | 0.143 | 0.032 | 0.225 | 0.045 | 0.053 | 0.012 | 0.119 | 0.020 |
| Bavaria | 0.263 | 0.032 | 0.344 | 0.044 | 0.150 | 0.011 | 0.237 | 0.019 |
| Saarland | −0.124 | 0.067 | −0.318 | 0.098 | 0.043 | 0.022 | −0.098 | 0.041 |
| Berlin | 0.268 | 0.043 | 0.289 | 0.057 | −0.420 | 0.016 | 0.131 | 0.023 |
| Brandenburg | −0.020 | 0.050 | 0.029 | 0.065 | −0.127 | 0.015 | 0.092 | 0.023 |
| Mecklenburg Western Pomerania | 0.016 | 0.064 | 0.148 | 0.081 | −0.214 | 0.020 | 0.035 | 0.030 |
| Saxony | 0.067 | 0.041 | 0.175 | 0.054 | −0.152 | 0.013 | −0.022 | 0.021 |
| Saxony-Anhalt | 0.058 | 0.048 | 0.078 | 0.062 | −0.131 | 0.015 | −0.077 | 0.024 |
| Thuringia | 0.107 | 0.054 | 0.120 | 0.072 | −0.039 | 0.016 | 0.057 | 0.025 |
| District unemployment rate | −0.031 | 0.008 | −0.002 | 0.011 | 0.040 | 0.003 | 0.038 | 0.004 |
| District long-term unemployment rate | −0.044 | 0.015 | −0.090 | 0.020 | −0.109 | 0.005 | −0.127 | 0.008 |
| District vacancy-unemployment ratio | 0.605 | 0.110 | 0.079 | 0.160 | 0.713 | 0.041 | 0.752 | 0.069 |
| Partner sanction (0-90 days) | 1.159 | 0.050 | 1.028 | 0.052 | - | - | - | - |
| Partner sanction (90+ days) | 0.996 | 0.034 | 1.075 | 0.035 | - | - | - | - |
| Own sanction | - | - | - | - | 0.145 | 0.012 | 0.158 | 0.026 |
| Partner sanction | - | - | - | - | −0.078 | 0.018 | 0.001 | 0.022 |

*Notes:* Joint estimation for both partners (P = 6). First, second, fifth and sixth column refer to men; the others to women. The number of couple spells is 356,510 and the log-likelihood = -1,993,468.

Table A3: Own and partner sanction effects on the transition rate to employment: heterogeneous effects

|  | Coefficients and standard errors | |
|  | Men | Women |
|---|---|---|
| Any children aged 0 to 2 | 0.1 *** | -0.225 *** |
|  | (0.011) | (0.021) |
| Any children aged 3 to 5 | 0.04 *** | -0.203 *** |
|  | (0.009) | (0.017) |
| Any children aged 6 to 17 | 0.051 *** | 0.006 |
|  | (0.009) | (0.016) |
| Own sanction | 0.27 *** | 0.408 *** |
|  | (0.042) | (0.077) |
| Own sanction (90+ days) | -0.1 *** | -0.261 *** |
|  | (0.028) | (0.058) |
| Partner sanction | 0.048 | 0.077 |
|  | (0.051) | (0.067) |
| Partner sanction (90+ days) | -0.191 *** | -0.00037 |
|  | (0.04) | (0.051) |
| Own sanction x any children aged 0 to 2 in household | 0.014 | -0.511 ** |
|  | (0.035) | (0.207) |
| Own sanction x any children aged 3 to 5 in household | 0.07 ** | -0.075 |
|  | (0.032) | (0.102) |
| Own sanction x any children aged 6 to 17 in household | 0.101 *** | 0.061 |
|  | (0.027) | (0.056) |
| Partner sanction x any children aged 0 to 2 in household | -0.111 | -0.41 *** |
|  | (0.085) | (0.088) |
| Partner sanction x any children aged 3 to 5 in household | -0.024 | 0.21 *** |
|  | (0.055) | (0.067) |
| Partner sanction x any children aged 6 to 17 in household | 0.155 *** | 0.177 *** |
|  | (0.038) | (0.047) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,337. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table A4: Partner sanction effect on the transition rate to the own sanction: heterogeneous effects

|  | Coefficients and standard errors | |
|---|---|---|
|  | Men | Women |
| Any children aged 0 to 2 | 0.1 *** | -0.225 *** |
|  | (0.011) | (0.021) |
| Any children aged 3 to 5 | 0.04 *** | -0.203 *** |
|  | (0.009) | (0.017) |
| Any children aged 6 to 17 | 0.051 *** | 0.006 |
|  | (0.009) | (0.016) |
| Own sanction | 0.27 *** | 0.408 *** |
|  | (0.042) | (0.077) |
| Own sanction (90+ days) | -0.1 *** | -0.261 *** |
|  | (0.028) | (0.058) |
| Partner sanction | 0.048 | 0.077 |
|  | (0.051) | (0.067) |
| Partner sanction (90+ days) | -0.191 *** | -0.00037 |
|  | (0.04) | (0.051) |
| Own sanction x any children aged 0 to 2 in household | 0.014 | -0.511 ** |
|  | (0.035) | (0.207) |
| Own sanction x any children aged 3 to 5 in household | 0.07 ** | -0.075 |
|  | (0.032) | (0.102) |
| Own sanction x any children aged 6 to 17 in household | 0.101 *** | 0.061 |
|  | (0.027) | (0.056) |
| Partner sanction x any children aged 0 to 2 in household | -0.111 | -0.41 *** |
|  | (0.085) | (0.088) |
| Partner sanction x any children aged 3 to 5 in household | -0.024 | 0.21 *** |
|  | (0.055) | (0.067) |
| Partner sanction x any children aged 6 to 17 in household | 0.155 *** | 0.177 *** |
|  | (0.038) | (0.047) |

*Notes:* Joint estimation for both partners (P = 6). The number of couple spells is 356,510 and the log-likelihood = -1,993,337. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table A5: Own and partner sanction effects on the transition rate to employment: single spell data

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Own sanction (0-90 days) | 0.307 *** | 0.437 *** |
| | (0.041) | (0.078) |
| Own sanction (90+ days) | 0.208 *** | 0.156 ** |
| | (0.041) | (0.07) |
| Partner sanction (0-90 days) | 0.105 ** | 0.148 ** |
| | (0.049) | (0.067) |
| Partner sanction (90+ days) | -0.089 ** | 0.136 ** |
| | (0.043) | (0.062) |

*Notes:* The estimation includes the first spell for each couple only and excludes subsequent spells of a couple in the sampling window. Joint estimation for both partners (P = 6). The number of couple spells is 315,537 and the log-likelihood = -1,761,999. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level

Table A6: Partner sanction effect on the transition rate to the own sanction: single spell data

| | Coefficients and standard errors | |
| --- | --- | --- |
| | Men | Women |
| Partner sanction (0-90 days) | -1.303 *** | -1.484 *** |
| | (0.059) | (0.061) |
| Partner sanction (90+ days) | -1.785 *** | -1.899 *** |
| | (0.053) | (0.055) |

*Notes:* The estimation includes the first spell for each couple only and excludes subsequent spells of a couple in the sampling window. Joint estimation for both partners (P = 6). The number of couple spells is 315,537 and the log-likelihood = -1,761,999. Control variables are included. ***, **, * indicate significance at the 1 %, 5 %, 10 % level